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Round Table

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THE SCIENTIST AND THE ATOM

n NBC Radio Discussion by HERBERT ANDERSON
HARRISON BROWN, WILLARD LIBBY
and CYRIL SMITH

Including

INSTITUTES FOR THE STUDY OF BASIC SCIENCE

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CONTENTS

Around the Round Table . . .

HERBERT L. ANDERSON, professor of physics, Institute of Nuclea Studies, University of Chicago

HARRISON BROWN, associate professor of chemistry, Department c Chemistry and Institute of Nuclear Studies, University of Chicago; autho of *Must Destruction Be Our Destiny?*

WILLARD F. LIBBY, professor of chemistry, Department of Chemistry an Institute of Nuclear Studies, University of Chicago

CYRIL S. SMITH, professor of chemistry, director of the Institute for th Study of Metals, University of Chicago; formerly research metallurgis American Brass Company; chief metallurgist, Atomic Bomb Project Los Alamos

Institutes for the Study of Basic Science 1

THE UNIVERSITY OF CHICAGO ROUND TABLE

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897th Broadcast in Cooperation with the National Broadcasting Compa

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The Scientist and the Atom

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MR. BROWN: Last Wednesday the new eleven-million-dollar building hich houses the University of Chicago's institutes for basic research as formally opened. The building, which is directly across the street om the site where the first self-sustaining nuclear chain reaction took ace, contains research facilities for the Institute for the Study of etals, the Institute of Radiobiology and Biophysics, and the Institute r Nuclear Studies. The facilities of the institutes include a large clotron, a betatron, equipment for obtaining extremely low temperures, and an electron microscope, in addition to general laboratory cilities. Over three hundred workers are engaged in research and sociated activities.

Gathered with me today are three men who have played important les in the building of these institutes: Cyril Smith, director of the stitute of Metals, formerly in charge of metallurgy for the Los Alaos atomic-energy project; Willard Libby, professor of chemistry in e Institute for Nuclear Studies; and Herbert Anderson, professor of hysics and the man primarily responsible for the design and construcon of the cyclotron, of which the University is so justly proud.

Smith, you have been intimately associated with the research instiutes since their founding about five years ago. Tell us a little about e way in which they were formed.

MR. SMITH: Five years ago most of us were hard at work on the omic bomb. Everybody knows the results of that work. From the andpoint of the scientists' work on this, one of the most interesting spects of it was to show the great value of cooperation among people different fields. And, as we started to think about work after the ar, we realized that many of the interesting scientific subjects were ose on the border lines of established fields—topics spreading over hysics and chemistry and many other things. We realized that it ould be desirable to have a research system set up so that it would e possible for people to get together, to share their ideas, mutually to imulate each other, and to have available equipment on a scale which

would be quite impossible for people working within a given depar ment.

MR. BROWN: These research institutes are called the institutes fo basic research. Perhaps we could give a few examples of the kind o work which is going on. We have within the institutes biologist physicists, and chemists, and we have all these nice facilities. What ar examples of some of the research work which is now going on withi the institutes?

MR. ANDERSON: It is an interesting fact that the development of nu clear physics of yesterday is being applied in the fields of chemistry biology, geology, and astronomy today. In our institutes we have me who are interested in the origin of the universe, the origin of the earth the origin of cosmic rays. All these fields have been able to be unde stood better through our understanding of the nucleus. So, we fin Harold Urey interested in the origin of the earth and applying the ne understanding of the radioactivity of potassium and argon in his studi of the earth. You, Libby, are interested in applying the radioactivity o carbon in the study of the history of man's development.

MR. LIBBY: Following Anderson's lead, I might continue by sayin that Professor Urey has developed a method of measuring the tempe atures which existed on earth in past times by measurement of the iso topic composition or atomic weight of the oxygen in sea shells whic were deposited in those past times. This is the first time that we hav had quantitative information about temperatures, and the results shou be of the greatest importance in geology.

Another example is some work with which we have been concerne in the use of the radioactive carbon which is generated in the earth b the cosmic radiation. It makes everyone and everything containing ca bon radioactive, and the radioactivity persists so long as the person o thing lives. At death it proceeds to disappear at an immutable rat and the measurement of the radioactivity of dead organic materia apparently gives time elapsed since death, or the age of the materia

MR. SMITH: You people seem to be talking about things of mo cosmic importance than concern me and the Institute for the Stud of Metals. In the field of metals we are not really concerned with astr nomical things or with things billions of years ago. But we are co cerned merely just in building up the fundamental understanding what makes metals useful. We have not aimed at all at developing

netal for given purposes, however; that is really the job of industry, ot of people in the academic world. We do not want to find out how o make a material for building jet engines or something of that kind, mportant though that is. We want merely to understand the laws vhich are common to all metals in order to provide a framework to e together work in all fields and to make advance by the practical eople easier.

And, of course, we spread very far from metals. The institute is called ne Institute for the Study of Metals, but a great deal of our work is n the ionic side, dealing with something like liquid helium, which is ery far from being a metal. In the case of heat treatment, for instance, vhich is extremely important to the practical steel metallurgist, we are oing some work which does not involve quenching steel and measur- ng its hardness, but we are studying the movement of the atoms which ccurs during heat treatment and other things. The most important ool which we are using is a very simple apparatus known as a torsion endulum, which measures the internal friction of the metal. This was eveloped originally as a means of studying the vibration dampening apacity of the metals and is a good illustration of how developments n one field will influence another.

MR. BROWN: One of the most interesting fields of research now going n in the institutes is over in the biology side. Professor Franck and his roup are working on the fundamental problems of photosynthesis, ttempting to learn just how plants manage to utilize the energy of he sunlight. Then there is the work which Professor Szilard is doing n microbiology. Szilard was a physicist who was in large part respon- ible for the atomic-energy project during the war; and at the end of he war he went into biology. I think his work provides a striking ex- mple of the interrelationship between fields which formerly were hought to be widely divergent.

What about some of these machines which exist over at the Institute? What about the cyclotron?

MR. ANDERSON: The Institute has built a number of machines. It has small cyclotron, a betatron, a Van de Graaff, and a kevatron; and last Wednesday it dedicated its new synchrocyclotron, a machine which ac- elerates protons to 450 million volts—the highest energy ever achieved rtificially in the laboratory. And this machine is capable of producing ew particles called mesons. The machines in general are used by the hemists and the radiobiologists, as well as by the metallurgists, for the

radioactive materials which they can make. These members of the i stitutes use the radioactive materials in their tracer studies, which pe mit them to understand, in detail, the chemical and biological process in which they are interested.

The physicist, on the other hand, has more or less turned his atte tion away from the problem of the structure of the nucleus, becau he has found, in the recent years, that the constituents of the nucle have a structure of their own, and he is devoting his attention to th structure of the constituents of the nucleus. For this purpose the lar synchrocyclotron is essential, since it produces the mesons, the particl which come out of the nuclear particles themselves.

MR. BROWN: Over in the Metals Institute you also have some machin of considerable interest, Smith. Could you tell us a little about those?

MR. SMITH: Actually when I hear the nuclear physicists boasting abo their huge machines, I sometimes think that in the Institute of Meta the little things are almost more important than the bigger ones. Th small torsion pendulum, which costs perhaps $25.00, has proved a ve useful tool. However, we do have a large installation for getting ve low temperatures—temperatures down to about one degree absolu so as to provide an environment for studying the properties of meta and other things. We have high-pressure equipment so that we can s what happens to metals under pressure up to 15,000–20,000 atmospher and so on.

MR. ANDERSON: I would like to say that we in nuclear physics are qu happy about having so close by a low-temperature laboratory such as provided in the Institute of Metals, since we have a great interest making use of the liquid hydrogen which is produced there.

MR. BROWN: I think that you made a very important point, Smi when you mentioned that there is much science which can be do without benefit of these large, very expensive machines. I know th there are several men in our institutes who are doing research on ve modest budgets and who are doing very important work indeed. Ho ever, I do think that all of us must realize that there are certain fro tiers of science, as, for example, the study of the fundamental nature matter, of mesons and what have you, which just cannot go on eas without the expenditure of two and a half million dollars or so, whi I believe was the cost of our big cyclotron.

It is very important for everyone constantly to remember that resear in science is constantly and unceasingly revolutionizing the condition

our lives. The work of these institutes, I think, is part of that revolution. Thus far we have been discussing basic research as such. Now let us spend some time discussing the way in which basic research eventually can end up with practical applications.

MR. ANDERSON: I would like to say that basic research is aimed at the understanding of the general laws of nature. These general laws are the base of our understanding of particular things. What we in nuclear physics would like to be able to do today is to achieve something analogous to Newton's laws for mechanics or Maxwell's equations for electrodynamics. We would like to find some equivalently simple law which would serve as a basis for all our understanding of nuclear phenomena.

MR. SMITH: In the metallurgical field I think that it is quite interesting to see the importance of Gibbs's phase rule, for instance. It would be extremely difficult to see why that should be useful; yet the metallurgist, knowing that certain things can happen and certain things cannot because of Gibbs's rule, which says what crystals or phases can exist in contact with each other, saves enormously in carrying out research. One has a framework in which to study new information, as it comes up, and in which to plan one's work.

MR. BROWN: A very interesting example as to how you can never predict with accuracy what will happen so far as a practical application is concerned happened here at the University of Chicago a number of years ago, when Dr. Huggins of our Medical School found a method for treating cancer of the prostate. There were several important ingredients to this treatment, which Huggins brought together, none of which had anything to do with cancer. There was one man, I believe in Switzerland, who was studying a chemical which was called phosphatase. There was another man who was studying something as remote from cancer as the seasonal cycle of the hedgehog. And there was still a third man in another country who was studying the lining membrane of the intestine. There was still another man who was studying certain biological aspects of bulls. Now, none of these individual researches in themselves had anything to do with cancer; and if they were done today, it is highly dubious that any organization set up to sponsor cancer research would have sponsored their work. Yet Dr. Huggins put these ingredients together, and out of it emerged a treatment which is a very good treatment indeed.

There is a British zoölogist by the name of John R. Baker who made a very nice statement once, I think, when he said that the linking-to-

gether of a toenail and an umbrella by a surrealist is scarcely more pr dictable than the linkings which result in some of the big discoveri in science.

In view of this, I think that we should discuss how industries and forth are interested in institutes such as ours.

MR. LIBBY: I would like to say that your example of Dr. Huggi research points out very clearly the practicality of basic research. W in our local institutes here at the University of Chicago, are primari concerned with the discovery of the general laws of nature, as And son says, and with the provision of a better understanding of natu phenomena.

But there is another aspect to our situation. We have a number sponsors—industrial and governmental—who visit us periodically, w engage in discussions of our mutual problems, and who have the opp tunity to take the new results of basic research and make technologi application of them.

The time lapse between the payoff of basic research and the time the research is extremely unpredictable, but I think that it is fair to s that the basic research does pay in terms of dollars and benefit to t general community in all cases in the end. I believe that one can ma an argument which is purely economic for supporting basic resear But there is, of course, an additional argument for basic research—t fundamental human desire to know more about the world and the u verse in which we live. Astronomy is an example of this. People are ve much interested in astronomical observations; but, fundamentally, o need not look to this basic human desire for an argument for the su port of basic research.

MR. ANDERSON: Would you not agree that there is some sort of divisi which exists here in that in the institutes where basic research is do the men are left alone to do their basic research, whereas the spons come in to see whether there might not be some application of this search? So, we leave for the sponsors the problem of applying the resu of our work, whereas we, in our turn, can concentrate on doing thi which have basic utility.

MR. SMITH: But I would like to point out that this element of unp dictability of the usefulness of basic research means that one has j one criterion as to what one does. The interests of the scientist see to be by far the best criterion to decide what one does. One does not t

he end result; one takes merely curiosity, or the aesthetic motivation f the scientists.

MR. BROWN: I believe that most scientists are motivated not by the spect of practical application but rather just by the desire to learn—the lesire to find new things. The scientist attempts to reduce the innumerble phenomena of nature down to their lowest common denominators; nd in that he gets a great deal of personal inner satisfaction.

Thomas Huxley expressed the inarticulate feelings of many scientists vhen he wrote that the research worker is inspired by the supreme deight of extending the realm of law and order ever further toward the nattainable goals of the infinitely great and the infinitely small, between vhich our little race of life is run.

MR. SMITH: But I myself feel that the scientist is interested actually n something more than law and order which can be specified as such. There is an element of order; but there is the form which goes through ll things. I think that the relation between a Mozart sonata, for instance, nd the scientist's enjoyment of the pattern of nature is interesting.

We have had a rather interesting example of that recently in my field. We found, for instance, that the shape of metal grains is patterned lmost exactly the same as the shape of biological cells, and a trough of oap bubbles is exactly the same thing. Here one sees the same fundanental principles of the relation of surfaces and shapes of things coming ut in totally different fields. It is really very beautiful to see these reations.

MR. ANDERSON: Of course, there is a general human desire to understand things. I think that this is one of the essential motivations of our nembers in the institutes. I like to think of the example of Rutherford, vho, in 1911, got the idea of the nuclear atom. His idea that the atom vas made up of electrons, surrounding or circulating around a tiny but nassive nucleus, is a very important and essential idea in the developnent of physics. When he got that idea, he could not and did not imgine the practical use which would be put to this conception. Neverheless, it is hard to imagine how all the developments of chemistry and nodern physics could have evolved if there had been a wrong idea of vhat an atom was like.

MR. BROWN: That is quite correct.

MR. LIBBY: I have an additional point about Lord Rutherford. I was eading just this morning a statement attributed to Lord Rutherford

as late as 1937 in which it is claimed that he said that he saw no practica application of his nuclear research. This, of course, was just two yea before Hahn's discovery of fission and all the consequences which fo lowed. It seems interesting that Lord Rutherford himself had no re conception of the imminence of the practical application of his r searches.

MR. BROWN: I am very much interested in a statement which Robe Hooke made many centuries ago which, I think, is indicative of th ramifications that basic research has had in ways other than purel practical. Hooke wrote a code of behavior for England's most distin guished scientific society, and he said the following: "That the busine and design of the Royal Society is to improve the knowledge of natur things in all useful arts, manufactures, mechanic practices, engine and inventions by experiments, but not meddling with divinity, met physics, morals, politics, grammar, rhetoric, or logic."

Now, Hooke's concept of science still exists in many quarters, I thin but who, upon looking over Hooke's list of forbidden subjects, can sa with clear conscience that science has not meddled with them? I do n think that there is a single concept involved in these subjects which r mains unaltered essentially as the result of basic research.

MR. LIBBY: I would like to return again to this little bit of researc with which we have been concerned on the dating of mummies to illu trate the point that the results of research are extremely unpredictab in their usefulness. The research consisted, in its original phase, in th investigation of the nature of the effects of cosmic rays on the eart Subsequent to the institution of this investigation, the thought occurr that this would indeed allow the dating of mummies. That is, the a cient times would be written on the organic materials remaining to th day. The method is a direct consequence of the purest researches of th cosmic-ray physicists and geochemists who have spent their lives tryin to understand the general makeup of the earth's atmosphere.

MR. BROWN: In our discussion of the functioning of the research ins tutes, we have left out one very important feature, and that is the trai ing of young men who themselves will eventually go out into the scie tific world.

MR. LIBBY: I would like to say that the university's role in the co munity's life is a unique one. The limiting factor at the present tim in the rate of technological expansion in this country is not money b

he number and quality of trained men available. It is the university vhich largely controls this all-important factor. One might therefore onclude that the training of men is a prime function of the university. We try to remember this in our local institute structure by recognizing nd supporting a program of instruction for advanced students in the ciences.

Mr. Brown: I think that we can summarize our discussion today in he following way: We agree that any technological development de- ends for its success upon the pieces of fundamental information accu- nulated, bit by bit, through the years by scientists the world over. No echnological problem can be solved, whether it be to build a new bridge, o build an atomic-energy plant, to cure cancer, to create a special new lloy, or to grow more crops without adequate reservoirs of funda- nental knowledge—knowledge which is, in general, accumulated with- ut thought of the problems to which the information might later be pplied.

The size of the reservoir of fundamental scientific information is per- aps the most important single feature which determines the extent o which man can control his environment.

New scientific discoveries make possible new technological develop- nents. As it is impossible to predict the nature of new scientific dis- overies, it is impossible to predict, with accuracy, the extent to which cience and technology properly applied can help in solving some of the nore urgent problems now facing the world. The best that can be done s to point to the developments which could take place on the basis of knowledge which already exists.

It is possible, however, to make one important generalization. The roblems now facing the world will not be solved in a satisfactory nanner in the absence of vigorous quests for new knowledge in all the ields of science, ranging from nuclear physics to astronomy, from bac- eriology to zoölogy.

A survey of the basic problems which confront the world demonstrates hat, contrary to the beliefs held by many people, man has suffered and s suffering under the handicap not of too much knowledge but of far oo little knowledge. This is nearly as true in the physical and biological ciences as it is true in politics, government, and the broad fields of uman relationships.

INSTITUTES FOR THE STUDY OF BASIC SCIENCE*

THE atomic age will be the age of knowledge. The release of atomi energy and the new tools and techniques discovered during wartim research have opened up vast new fields of study. It is probable that th instruments, such as the atomic "pile," will be as important to moder science as the microscope was to the investigators of earlier days. If a our resources are used to the full, we should be able to expand ou knowledge of the basic laws and actions of our universe. With this ex panded knowledge, man should be able to establish an undreamed-o mastery over his environment.

For many years most Americans have regarded the United States a the scientific leader of the world. Actually, the great scientific achieve ments of this nation have been exceedingly few. In our laboratories w have had many inventors—but relatively few scientists who were able t increase our basic knowledge, few who have added to man's master and understanding of his environment. It is startling to learn that, du ing the some fifty years of Nobel prize awards for achievements i physics, chemistry, and medicine, only 26 were awarded to American European scientists have received 124, and German scientists alone hav been awarded 39. It is a startling fact that this country's great basic di coveries during the last two centuries can be counted on the fingers o the hands. The industrial achievements of the United States have rathe been made possible by a host of American inventors who have had th ability to invent, to modify, to improve, and to apply but not to mak the great discoveries. The vast majority of great basic discoveries whic have shaped our way of life have come, for the most part, from Euro pean laboratories. The radio was developed in America, but it is base upon the discoveries of Germans, Italians, and Englishmen. The bas discoveries for the development of the airplane, the automobile, the ele tric light, the atomic bomb—all American developments—we owe t fundamental discoveries made in European laboratories. Now that th European source of our strength has vanished, America must bolste its basic research agencies. The establishment of three institutes at th University of Chicago for the study of basic science is a step towar supporting basic research beyond the level heretofore contemplated i American institutions.

* Prepared from source materials at the University of Chicago.

It has always been the policy of the University of Chicago to support, in every possible way, the prosecution of basic research. During the last war, when the government was faced with the solution of basic scientific problems of unprecedented magnitude, the University was called upon for the study of some of these problems. The most important and the most difficult was the development of controlled atomic energy and a method for producing, extracting, and refining plutonium as an atomic explosive. Many of the best scientists in the world, including members of the regular University staff, worked together on this atomic project. On December 2, 1942, in a converted squash court underneath the West Stands on the University of Chicago's football field, the first self-sustaining nuclear chain reaction was achieved by man. This epic event, which ushered in the atomic age, climaxed the efforts of these dozens of great scientists now working together but who had been conducting basic atomic research for many years.

All the world knows of the terribly destructive power of atomic energy. The scientists who were there when the first chain-reaction pile was put into operation on the University of Chicago campus knew, better than anyone else, of the danger in the forces which they had set into motion. But they knew also of some of the possibilities of good for humanity which might flow from their work. They knew that (1) the discoveries of atomic science might cure disease; (2) atomic energy might provide undreamed-of power for industrial use; and (3) the unfolding new science might bring unimagined benefits to mankind.

These scientists thus resolved instinctively that their talents and energies would be used in the realization of the boundless promises for good which atomic discoveries would make possible.

The incredible complexity of nuclear research had made it imperative that the wartime problem be attacked by teams of scientists. One man could not be expected to have the mastery of physics, metallurgy, chemistry, mathematics, biology, physiology, and other sciences required to solve the atomic, metallurgical, and biological problems faced by these wartime researchers. The fundamental knowledge which they had discovered was sufficient to produce the atomic bomb. It was insufficient for practically every other purpose. Therefore, three days after the first bomb was exploded at Alamogordo, these scientists decided that, in the best interests of the country and of science, it would be highly desirable to continue to work together on the basic problems which their discoveries had posed. The desire of the scientists to work together grew out of the realization that, if scattered throughout the country, each in a

different research laboratory, their progress toward an understandin of the atomic, metallurgical, and biological problems resulting fron wartime research would be pitifully slow.

The officers and trustees of the University of Chicago expressed thei faith in the importance of this work and offered to cooperate by under writing the cost of making this continued research possible in an effor to assure that the discoveries of the new science may be for good rathe than for evil. To meet this need, the University of Chicago organize the Institute for Nuclear Studies, the Institute for the Study of Metal and the Institute of Radiobiology and Biophysics, all devoted to th study of basic science.

The Institute for Nuclear Studies

The broad program of this group of scientists is to conduct researc on the structure of the nucleus of the atom, on radioisotopes, and on th fundamental particles of matter, such as the neutron, the neutrino, an the meson. Because of present national policy, the Institute is not con ducting research on atomic power. It is altogether possible, however, tha the fundamental studies now in progress will ultimately have a wide application than would the refinement of atomic-power processes.

The research to be conducted requires the availability of high-energ particles. To produce these beta rays, photons, protons, deuterons, neu trons, and accelerated nuclei, the Institute is equipped with a 100-million electron-volt betatron and a 350–400-million-electric-volt synchrocycl tron. Together with a smaller cyclotron, these instruments will be use to produce very high-energy particles in narrow beams. When larg concentrations of neutrons of low and intermediate energies are neede research workers continue to request the use of the chain-reactor "pil at the Argonne National Laboratory.

One group of problems under investigation involves the use of isotop Carbon 14. This is one of the most important isotopes in biological an industrial research because of its use as a "tracer." Radioactive isotop are tools which are now available in large quantities. These forms elements constantly send off rays of energy. They can be combined wi other elements to form compounds; and the compounds, because of the radiation, can always be identified. Radioactive carbon, for example, ca be used to form glucose; and when the glucose is swallowed, its cour through the body can be accurately followed. Their use, thus, may gi us the clue to many baffling life-processes. In industry these radioacti

tracers" are already being used to study chemical reactions and the role f catalysts.

Among the many other problems which are currently being investi-ated are: the adequacy of the present meson theory to explain the rela-ively long life of negative mesons in contact with matter; the use of adioactive tracers in the study of the mechanism of chemical reactions; study of the magnetic properties of the new man-made elements; a tudy of the magnetic moments of some light nuclei; precise studies of he energies of the emitted particles and of the recoils occurring in nu-lear reactions; the asymmetrical splitting of the nucleus if we assume hat the nucleus is shaped like a liquid drop; the effect of temperature pon the fixation of isotopic oxygen into calcium carbonate; a study of he mechanism of the quenching action of organic vapors in Geiger ounters; attempts to verify the existence of, and identify if found, the ositive particles which have been reported in connection with emission f negative electrons from radioactive nuclei; a search for emitters of lpha particles among naturally occurring heavy nuclei; studies of the arer fission products and of the chemical nature of delayed neutron mitters in fission; investigations of chemical reactions which take place fter a radioactive transformation of one of the atoms in a molecule; roblems concerned with the formation of the solar system; the process f ultra-microanalysis and the origin of cosmic rays and the origin of lements.

The Institute for the Study of Metals

The personnel of this institute is made up of metallurgists, physicists, nd physical chemists who are devoting their whole attention to the tudy of metals. In general, the research is aimed at a basic understand-ng of the nature of the solid state. Heretofore, the study of metals has een largely in the hands of industrial metallurgists who have been oc-upied almost exclusively with the study of the problems of their own pecific industry and thus unable to give attention to the more funda-ental problems of the relationship between metallic properties and tructure. Furthermore, the physics of the solid state, up to the last few ears, has not been in an advanced enough state to suggest cooperation etween those fields which bear upon this subject. Now sufficient ad-ances have been made to warrant this cooperative effort.

One of the most important fields of work has been the study of the echanical properties of metals, not in the engineering sense, but in erms of the micromechanism of the reaction of metals to stress. The

studies of relaxation of stress at grain boundaries and relaxation resu ing from diffusion of atoms to preferential attice positions throw lig on the structure of metals and are bound to have important practic results. The quantitative study of the effect of temperature on the b havior of metals provides the best check on the theoretical concep Work on microelasticity led to some understanding of one cause allotropic transformations and even permitted the prediction of the e istence of a new modification of one metal—lithium—that was th found by investigation at liquid-air temperatures. Another importa field of investigation is that of grain growth, a factor of great impo tance in almost all metal products, and many related studies of the stru ture and properties of grain and interphase boundaries.

Equipment has been installed to produce liquid hydrogen and liqu helium for use as refrigerants for studies of metals at low temperatu (—440° F.). The laboratory is making studies at low temperatures heat capacity, thermal and electrical conductivities, magnetic suscep bilities, and of superconductivity and mechanical behavior of metals low temperatures. There is an excellently equipped X-ray diffracti laboratory; and equipment is available for determining the structu and properties of metals under high pressure and for carrying out si ple diffusion and transformation experiments at pressure up to 150,0 pounds per square inch.

The Institute of Radiobiology and Biophysics

The work of this institute is aimed at the fundamental study of t effects of radiation on all biological systems, whether those systems nucleoproteins, genes, bacteria, virus, muscle tissue, nerves, or abnorm systems, such as cancer. Science now has several new tools with whi it can explore the processes of biological growth. Abundant quantit of alpha rays, beta rays, gamma rays, and neutrons may not only be u ful in themselves (the therapeutic value of these high-speed radiati in cancer treatment, for instance, is still incompletely known) but a their use should allow us to gain a better understanding of the c trolling factors in all biological systems.

In addition to an investigation of the effects of ionizing radiations tissue, the Institute is studying: (1) the nature of the mutation proc in viruses; (2) biochemical mutations, especially with respect to phc synthesis; (3) the synthesis and nature of nucleic acid; (4) the electri nature of the nerve impulse and the relation of chemical reactions

ctrical phenomena; and (5) the effects of a microbeam of protons fractions of a cell nucleus.

Basic Research and the Institutes

Under federal law the government is given a monopoly in the pro- ction of atomic energy as such and in the production of fissionable aterial—plutonium, for example. But other knowledge coming from is new science is available to everyone who has the ability to use it.

The Atomic Energy Commission is directed by law to encourage pri- te research. It was recognized by our lawmakers that complete govern- ent domination of every aspect of this great new economic fact would use a basic change in our economy. On the other hand, freedom of search cannot be maintained if the nature of the investigations is dic- ed only by specific industrial needs. The great contribution which the stitutes can make is in the field of pure research.

The discoveries which are sure to come from the concerted efforts of entists working together in such a cooperative set-up are of vital im- rtance for several reasons. Knowledge about the fundamental forms matter, about metals, and about biological systems may completely ange our way of life. Research is the key to better living. The dis- veries of electricity, steam power, radio waves, X-rays, bacteria, and e laws of aerodynamics have had profound effects upon our civiliza- n. Such changes are often accompanied by revolutions in our indus- al organizations. The discoveries which may come from atomic re- arch will certainly have a great impact upon the activities and oper- ions of our corporations. The discovery, for example, of a cheap metal hose electrical resistance is less than that of copper would necessitate eat changes in our electrical and mining industries. While the major scoveries will probably not have an equal impact upon all industries, sic scientific research is important to all.

The three Institutes meet a need which cannot be met by private dustry, by government research laboratories, or by many colleges and iversities of the country. Our great industrial laboratories must, of cessity, be concerned with the problems of producing a better product less cost. Such laboratories have little, if any, time to study broad basic oblems. Ordinarily their work must be directed by the demand for sults which are clearly practicable.

Only in a university which is devoted to basic research is knowledge, hether or not it has any apparent application, recognized as a worthy sult of labor. And, yet, most of the great discoveries which have

changed our lives were made by men who hardly suspected that the work would have any industrial value. As late as 1937, Lord Ruthe ford, one of the pioneers of atomic science, stated his belief that h studies of atomic energy would never have any practical value.

Basic research is independent research. It is tremendously expensiv research. Corps of scientists in widely differing fields must be associate with one another if maximum results are to be achieved. They must b furnished with quantities of expensive and complicated equipment. Th one thing which the war taught is that research cannot be carried ou with the test tube alone.

The purpose of basic research is to increase knowledge. New know edge often changes our way of life more profoundly than do intern tional power politics and military forces. Faraday, for example, in h search for knowledge about the basic nature of electricity, incidental laid the basis for our entire electrical industry. Upon Boyle's discoveri of the laws governing the expansion of gases we have built a tremendou transportation industry. Hertz's inquiry into the nature of radio wav has had an equally important effect upon our industrial economy.

American industry is now dependent upon this type of basic researc Before the war it drew upon a vast stockpile of fundamental knowledg as has been pointed out—almost all of it the product of European labor tories. The startling developments during the war were possible becau of this unapplied knowledge. Our stockpile has been diminished an in some areas, completely exhausted. Unless we wish to run the risk industrial stagnation, our basic knowledge must be increased. The bas knowledge of today is the industrial knowledge of tomorrow. Wi new knowledge, American enterprise can maintain its leadership an forge ahead. New organizations, giving vitality to our economy, can formed.

Further, laboratories for basic scientific research must operate com pletely free of all types of pressure or influence. Freedom of resear is certainly essential to freedom of enterprise. If shackles—however lig they might appear—were placed upon our basic scientific investigato the progress of all industrial sciences would be retarded. These Ins tutes can be of maximum value only if they remain free of any and kinds of domination—from whatever source. This freedom can best maintained by avoiding dependence for financial support solely on t government, solely on industry, or solely on individual contributio All are playing a part.

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